SCIENCE:

A Weekly Record of Scientific Progress.

JOHN MICHELS, Editor.

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We are pleased to remark some prospect of renewed astronomical activity at the Dearborn Observatory, Chicago. This institution was, for a period of more than ten years, in possession of the largest refracting telescope in existence—the object glass of which has an aperture of eighteen and one-half inches. The great telescopes which have since been built, and are now in process of construction, have the apparent effect of dwarfing the Chicago telescope, which, at the time it was made, was a great advance on every thing that had preceded it. There seems to be the best of reason for doubting, however, whether any other instrument at present in existence is surely superior to the Chicago refractor for efficient astronomical work. Mr. S. W. Burnham, distinguished for his researches in double stars, speaks with authority in this matter—"I know of no object, faint or otherwise, which has been seen at Washington or elsewhere, that cannot be seen perfectly here [at Chicago] and accurately measured." Professor Newcomb, in his "Uranian and Neptunian Systems, Investigated with the 26-inch Equatorial of the United States Naval Observatory, Washington," remarks that Ariel and Umbriel, the inner satellites of Uranus, "are visible only when the atmosphere is very fine, and are then difficult objects," and considers it very doubtful whether these objects have ever been seen with an aperture so small as twelve inches. Director Hough, of the Dearborn Observatory, states that near the time of the planet's opposition, these satellites can readily be seen and measured, under ordinary atmospheric conditions, with the Chicago telescope. If, as is quite possible, the Chicago refractor should prove to be quite as effective in actual observation as some of the larger telescopes of a later day, we shall have another of those instances frequently forced upon the astronomer, wherein his computation of the adequacy of a particular instrument does not tally with its observational effectiveness. Every astronomer, then, must regret that so competent an instrument must, through lack of endowment, be lying mainly idle, or, at the most, only employed by those who are able to turn it to scientific observation without pecuniary compensation. The valued work of Mr. Burnham with this instrument, in the discovery and observation of double stars, is well known. Professor Hough, in connection with Professor Colbert, conducted a series of observations of Jupiter at the late opposition. Owing to the discordance in the determinations of the ellipticity of the planet's disk from observation, their attention was given to a new determination of this quantity, with these results:

By Professsr Hough	1—16.23
By Professor Colbert	1—16.73

The English Nautical Almanac uses the value 1–13.71, while the value 1–16.40 is adopted in the American Ephemeris. With the same magnifying power, 638 diameters, the absolute polar and equatorial diameters of the planet were observed to be, for the mean distance of Jupiter from the Sun:

	Polar.	Equa'l.
By Professor Hough	36".319	38".704
By Professor Colbert	36".030	38".316

Assuming a solar parallax of 8".81, the measures of Professor Hough give for the equatorial diameter 90,570 miles, and for the polar diameter 85,000 miles.

Measures of the angle of position of the north edge of the equatorial belt show that it had the same direction around the entire circumference, and that this direction (exactly parallel to the planet's equator) was maintained throughout the entire opposition. Very complete measures of the apparent latitudes and widths of the several components of the belt system of Jupiter were also made, the great red spot co-inciding very nearly with one of these belts. The reduced measures of apparent latitude show very clearly that the belts were arranged symmetrically on either side of the equator, three being in the northern and three in the southern hemisphere of Jupiter. The report on these observations is accompanied with wood-cuts showing the red spot, the belt system, and, to some extent, the structure of the great equatorial belt. From the observations of this spot, Professor Colbert has computed the time of rotation of the planet on its axis: he finds it to be 9h. 55m. 34.2s., differing about eight seconds from the value hitherto considered the most probable.

Micrometric measures of the diameters of the four satellites of Jupiter were made on three nights, the resulting values being, at mean distance of the planet:

I.	II.	III.	IV.
1".114	0".980	1".778	1".457

The actual diameters of the satellites given by these measures are 2610, 2290, 4160, and 3410 miles, respectively.

But the superior quality of the object glass of the Chicago refractor is more effective with such objects as the satellites of Uranus; micrometric observations were secured as follows:

Of Ariel, on four nights.
Of Umbriel, on one night.
Of Titania, on eight nights.
Of Oberon, on seven nights.

And this, notwithstanding that the observations were begun late in the opposition, and were interrupted by an unusual amount of cloudy weather. We should like to see the superior light-gathering power of this object glass turned toward systematic figuring of the fainter nebulæ.

We may mention the meridian circle of the Dear-

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in conjunction with the negative or lower end of the needle. This is continued from high to low and from low to high water and from day to day, the result being recorded as read off. The mechanical difficulties in the construction of the machine are very great, but not considered insuperable.

ON THE DEFICIENCIES OF METEOROLOGICAL WORK IN DATA OF VALUE TO AGRICULTURE, AND MEANS FOR SUPPLYING THEM.

By WILLIAM MCMURTRIE.

Meteorological records, as they are and have been and are being made, are deficient in many of those data which have the most important influence upon farm crops. Temperatures are recorded, but they are always observed in the shade. Rainfall is given, but often in such a way as to render its record of no value in the study of the development and condition of crops, because no indication is given as to the way in which it is distributed ; light being of little importance to meteorologists generally, while it is one of the most potent factors in the development of vegetable and animal life, has been almost completely ignored. Late investigations have proven conclusively the importance of the tension of atmospheric electricity upon vegetation, and it should be regularly observed and recorded. In fact, meteorologists have principally confined themselves to the record and study of such conditions as enable them to predict the approach and occurrence of storms, thus looking more to the commercial than to the cultural side. Gasparin was the first to call attention to the importance of the relations of Meteorology to agriculture, and he has had at least two active followers—Quetelet in Belgium, and Marie Davy in France. Through the instrumentality of the latter there has been established, near Paris, an observatory of Agricultural Meteorology, where observation and record of all the conditions above named is made. The results already obtained have shown great practical value, and worthy of the means and labor required in securing them. In this country we have nothing similar to it. Our Signal Bureau, as nearly perfect as may be for the purposes for which it was designed, is devoted to the record and study of those observations as will render possible the prediction of future conditions which may affect human affairs, than such as may influence the development of crops. Besides this, the number of stations at which observations are made in this country is too limited, being not over 800, while for agricultural work 3,000 would not be excessive. Additional work should, therefore, be carried on, and observations at a larger number of stations made and recorded, to be discussed in connection with the records of observations made upon the condition of the crops. The nature of the work is such that it should be undertaken by the Department of Agriculture, and the organization of the latter with the 2,300 reporters it already employed would be well adapted to it. Fortunately, General Le Duc, the Commissioner of agriculture, is in favor of the establishment of such work in the Department, but will require congressional support to enable him to do so. The plan of work suggested by the author is as follows: 1. The establishment of a system of observation and record among the reporters of the Department of Agriculture, and others whose co-operation could be secured throughout the United States and Territories, with instructions to observers to keep careful records of the conditions of atmospheric pressure, temperature in its various relations, relative humidity, evaporation of moisture, winds, light, tension of atmospheric electricity, occurrence of dews, fogs and frosts, and report them at stated intervals of time to the Department for consideration and permanent record. I. The collection of meteorological records from every part of the world, from which to construct detailed tables showing the relations of all the conditions named above, and may influence the growth and health of vegetation. 3. The construction of maps showing the geographical distribution of crops, to be used in connection with the meteorological or climatic data to be collected.

PRELIMINARY ACCOUNT OF A SPECULATIVE AND PRACTICAL SEARCH FOR A TRANS-NEPTUNIAN PLANET.

By D. P. TODD, M. A., Assistant in the Office of the American Ephemeris and Nautical Almanac.

So early as the year 1834, HANSEN was credited with expression of the opinion, in correspondence with the elder BOUVARD, that a single exterior planet would not account for the differences between the tabular and observed longitudes of the planet Uranus. Dr. GOULD, however, in his "Report on the History of the Discovery of Neptune," says: "I have the authority of that eminent astromener himself (HANSEN) for stating, that the assertion must have been founded on some misapprehension, as he is confident of never having expressed or entertained that belief."

Professor PEIRCE's criticism of the investigations of LE VERRIER, to the effect that his predicted orbit of Neptune was so widely discordant from its observed orbit as to indicate that his computations did not pertain to the actual disturbing planet, elicited from him the reply that the perturbations of Uranus due to a possible planet exterior to Neptune might readily cause an uncertainty of $5''$ to $7''$ in the fundamental data of his research.

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Sometime in the spring of 1874, the first preliminary outline of the very simple method which I have here employed in the treatment of planetary residuals with reference to exterior perturbation, suggested itself to me. For more than three years very little opportunity offered for consideration of the problem of a trans-Neptunian planet. In August, 1877, however, I began to devote the larger portion of my leisure time to the theoretic side of the question. It was soon evident that no certain hold upon any possible cause of exterior perturbation could be obtained from the residuals of Newcomb's tables. And I may remark here that I have consequently chosen the term *speculative* rather than *theoretic* as applying more fitly to the investigation which preceded the actual telescopic search.

It did not seem to me that the magnificent researches of Le Verrier and Adams on the perturbations of Uranus should be taken as models in the present investigations, for two reasons :

(1) The residuals of longitude which must form the basis of the investigation are not sufficiently well marked to justify the execution of so laborious a research, especially if it be found that a simple, rational treatment, unencumbered with the refinements of analysis, may be fairly interpreted as indicating the position of an exterior perturbing body with merely a rough approximation.

(2) Even in the case of Uranus, and the theoretic search for Neptune, where the residuals of longitude were very strongly marked, many of the elements pertaining to the disturbing planet, which Adams and Le Verrier sought to determine theoretically, turned out afterward, when their real values became known, to have been indicated with only meagre precision. Much less should we now expect these elements to be given with any certainty in the case of a planet exterior to Venus.

This provisional treatment of the residuals of Uranus was undertaken, then, as a preliminary to the proposed

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ON THE DEFICIENCIES OF METEOROLOGICAL WORK IN DATA OF VALUE TO AGRICULTURE, AND MEANS FOR SUPPLYING THEM.

By WILLIAM McMURTRIE.

Meteorological records, as they are and have been and are being made, are deficient in many of those data which have the most important influence upon farm crops. Temperatures are recorded, but they are always observed in the shade. Rainfall is given, but often in such a way as to render its record of no value in the study of the development and condition of crops, because no indication is given as to the way in which it is distributed ; light being of little importance to meteorologists generally, while it is one of the most potent factors in the development of vegetable and animal life, has been almost completely ignored. Late investigations have proven conclusively the importance of the tension of atmospheric electricity upon vegetation, and it should be regularly observed and recorded. In fact, meteorologists have principally confined themselves to the record and study of such conditions as enable them to predict the approach and occurrence of storms, thus looking more to the commercial than to the cultural side. Gasparin was the first to call attention to the importance of the relations of Meteorology to agriculture, and he has had at least two active followers—Quetelet in Belgium, and Marie Davy in France. Through the instrumentality of the latter there has been established, near Paris, an observatory of Agricultural Meteorology, where observation and record of all the conditions above named is made. The results already obtained have shown great practical value, and worthy of the means and labor required in securing them. In this country we have nothing similar to it. Our Signal Bureau, as nearly perfect as may be for the purposes for which it was designed, is devoted to the record and study of those observations as will render possible the prediction of future conditions which may affect human affairs, than such as may influence the development of crops. Besides this, the number of stations at which observations are made in this country is too limited, being not over 800, while for agricultural work 3,000 would not be excessive. Additional work should, therefore, be carried on, and observations at a larger number of stations made and recorded, to be discussed in connection with the records of observations made upon the condition of the crops. The nature of the work is such that it should be undertaken by the Department of Agriculture, and the organization of the latter with the 2,300 reporters it already employed would be well adapted to it. Fortunately, General Le Duc, the Commissioner of agriculture, is in favor of the establishment of such work in the Department, but will require congressional support to enable him to do so. The plan of work suggested by the author is as follows: 1. The establishment of a system of observation and record among the reporters of the Department of Agriculture, and others whose co-operation could be secured throughout the United States and Territories, with instructions to observers to keep careful records of the conditions of atmospheric pressure, temperature in its various relations, relative humidity, evaporation of moisture, winds, light, tension of atmospheric electricity, occurrence of dews, fogs and frosts, and report them at stated intervals of time to the Department for consideration and permanent record. I. The collection of meteorological records from every part of the world, from which to construct detailed tables showing the relations of all the conditions named above, and may influence the growth and health of vegetation. 3. The construction of maps showing the geographical distribution of crops, to be used in connection with the meteorological or climatic data to be collected.

PRELIMINARY ACCOUNT OF A SPECULATIVE AND PRACTICAL SEARCH FOR A TRANS-NEPTUNIAN PLANET.

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This provisional treatment of the residuals of Uranus was undertaken, then, as a preliminary to the proposed

telescopic search to determine whether that search was worth undertaking, and if so, at what point approximately it was best to begin.

I.—We now consider, *seriatim*, the errors of the elements of the perturbed planet—errors which the very hypothesis of a disturbing body introduces, and which must have entered into the tables of the inferior planet, as constructed independently of unknown exterior perturbation. We consider what the effect of these errors may be, and how far it may be eliminated or subtracted from the residuals of the actual theory of the planet. These residuals are, of course, first corrected for any known error of theory or tables, or erroneous masses of known perturbing planets.

(1) *The error of mean distance of the perturbed planet.*—Any error of radius vector enters very largely into the residuals of heliocentric longitude, if the observations are made at any considerable intervals from the planet's opposition. If it is suspected that the error of radius vector will vitiate the residuals of longitude, we may avoid its effect by passing to residuals of geocentric longitude. Or we may confine our research to the mean residuals of observations near the opposition points, and symmetrically placed with reference thereto. The effect of erroneous radius vector is thereby eliminated.

(2.) *The error of periodic time of the perturbed planet.*—If the residuals are examined graphically, the eye will readily detect whether any correction to the periodic time is advisable. If, in general, the mean line of the residuals is nearly a right line, and makes a given angle with the line of zero-residual, it may fairly be concluded that the residuals need a correction depending directly on the time, the magnitude of the co-efficient of which is indicated by the divergence of the two residual-lines.

I had considered the problem only thus far when it occurred to me to apply the method, only partially developed, to the determination of an approximate position of Neptune from the residuals of Bouvard's Tables of Uranus, published in 1821. Taking also the residuals from observations up to 1824, and not permitting myself a knowledge of the longitude of Neptune at any epoch, a very little labor gave me an approximate position of the disturbing planet from which, it now appears, Neptune might easily have been found some twenty years in advance of its actual discovery.

When my work had advanced to this stage, a mere chance threw in my way a copy of Sir John Herschel's *Outlines of Astronomy*, (which I had never before examined): I at once observed that my treatment of the residuals of Uranus with reference to a planet exterior to Neptune was quite similar to his "dynamical" exposition of the perturbations of Uranus arising from Neptune itself. And I was farther gratified to find that he had given a very full and lucid statement of the effect upon the longitude-residuals caused by errors of the third and forth elements of the perturbed planet—the error of eccentricity, and the error of longitude of perihelion.

(3.) *The error of eccentricity of the perturbed planet.*—(See Sir John Herschel's *Outlines of Astronomy*, page 536.

(4.) *The error of longitude of perihelion of the perturbed planet.*—(Ibid., page 537.)

When the longitude-residuals have been corrected in this manner, we proceed on the assumption that any outstanding residuals are due to unexplained exterior perturbation.

II.—Of the seven elements of the disturbing planet, we must assume a value of one: the values of three others, together with the mass of the disturbing planet, we may consider as theoretically determinable from the longitude-residuals themselves.

(1.) *The mean distance of the disturbing planet.*—Regarding the next order of distance beyond Neptune as occupied by the planet for which we are searching, I assumed, as a first value of mean distance, a=46.0: this value seemed to be indicated by a fair induction. The periodic time of the planet would then be 312 years, and conjunctions with Uranus would occur nearly at intervals of 115 years.

(2.) *The eccentricity of the disturbing planet.*—Even with the large residuals of Uranus employed in the investigations of Le Verrier and Adams, the derived value of the eccentricity of Neptune was entirely illusory. The several values of eccentricity of Neptune resulting from their investigations are as follows:

Adams (*first hypothesis*)	0.16103
Le Verrier	0.10761
Adams (*second hypothesis*)	0.120615

The eccentricity given by investigation of the orbit of Neptune from observations of the planet was:

Newcomb (*Tables of Neptune*)	0.0089903

We should, therefore, expect nothing of any attempt to arrive at the eccentricity of an orbit exterior to that of Neptune.

(3.) *The longitude of perihelion of the disturbing planet.*—Much the same remark obtains in reference to this element. The several values of longitude of perihelion of Neptune, resulting from the researches in perturbations of Uranus, are as follows:

Adams (*first hypothesis*)	315° 57′
Le Verrier	284° 45′
Adams (*second hypothesis*)	299° 11′

The longitude of perihelion given by observations of the planet is:

Newcomb (*Tables of Neptune*)	46° 6′ 39″ .7

Evidently it would not be wise to include this element in the investigation.

(4.) *The epoch of the disturbing planet.*—If we can obtain even a rough approximation to the value of this element, the end of the investigation is fully attained. An inspection of the outstanding residuals, graphically exhibited, will show, without further labor, the epochs of maximum disturbance. We may prepare an approximate perturbative curve, the epochs of maximum disturbance of which shall be in harmony with the assumption of mean distance of the exterior planet. By applying this to the plot of outstanding residuals, we may decide at what points the application of the perturbative curve best accounts for them. The amount of excursion in its several sinuses we need not, for this purpose, attend to with any great care: this will depend upon the mass and distance of the disturbing planet; and, that it will be unavailing to attempt any determination of the mass in the present case will be evident from the fact that the mass of Neptune, from the theoretical investigations of LeVerrier and Adams, was widely discrepant:

Adams (*first hypothesis*)	0.0001656	$\frac{1}{6039}$
Le Verrier	0.0001075	$\frac{1}{9300}$
Adams (*second hypothesis*)	0.00015003	$\frac{1}{6666}$

While the most reliable mass of Neptune from observation was:

Newcomb (*motion of the satellite*)	0.00005160	$\frac{1}{19380}$

We thus have the inverse problem of perturbation reduced to a very simple rational form. The residuals of longitude of Uranus were next treated in accordance with this method.

In his *Investigation of the Orbit of Uranus*, Newcomb presents three series of residuals; the mass of Neptune finally adopted in the tables, $\frac{1}{19700}$, corresponds very nearly to the mean of the first and third series. But the mass of Neptune which was employed in this investigation is that given by Newcomb's discussion of the motion of the satellite of Neptune, and is $\frac{1}{19380}$. Our first step, then, was to correct these mean residuals into accordance with this adopted mass.

Afterward, examining these corrected residuals according to the method just related, in reference to unexplained perturbing action, I concluded that Uranus was in conjunction with an exterior perturbing body between the years 1780 and 1795, and that another conjunction would take place at some time before the close of the present century. The most probable position of the exterior planet I therefore considered to be about 170° of longitude; the probable error of the position I considered roughly 10°. This result was reached on the morning of the 10th of October, 1877. During the few days immediately following I reviewed this examination, as much as possible independently of the previous result, and at the same time varying the assumed mean distance. With a value of a = 52.0 (which I finally considered inductively the most probable) I set down the longitude of the exterior planet equal to 162° ± 6°. This result was reached on the evening of the 14th of October. I now turned my attention toward a similar treatment of the residuals of Neptune, with a slight hope of getting a confirmatory result. Two suppositions agreed in fixing the longitude at about 180° to 200°, respectively. I therefore, on some day in the latter part of

October, 1877, wrote down as the exposition of all my inquiry the following results:

EXTERIOR PLANET.—Longitude (1877.84), 170° ± 10°.
Mean distance from the sun, 52.0.
Period of revolution about the sun, 375 years.
Mean and daily motion, 9″.46.
Angular diameter, 2″.1.
Stellar magnitude, 13+.
Longitude of ascending node, 103°.
Inclination of orbit to ecliptic, 1° 24′.

If a new disturbing planet exists in the longitude here indicated, nearly a century must elapse before its existence can be asserted at all positively from the residuals of Neptune alone.

I should never have been able to execute the telescopic search consequent upon the investigation just related, had it not been for the courteous offices of Rear Admiral Rogers, Superintendent of the Naval Observatory, and Professor Hall, in charge of the great refractor. It was with this instrument—the 26-inch equatorial—that the search was conducted. It seemed to me that I should begin the search at a point about 20° preceding that indicated as the most probable position of the planet, and continue it to a point following by the same distance. But, a careful search extending over a zone of this length, and of sufficient width to be certain to contain the supposable planet, would be a work of such magnitude that I could not expect its completion under several years. I therefore had recourse to an inductive determination of the inclination and longitude of node of the planet's orbit.

I computed anew the position of the invariable plane of the solar system. A differential comparison of its inclination with the inclination of the orbits of the major planets, gave, with little uncertainty so far as the mere induction was concerned, the inclination of the orbit of the trans-Neptunian planet equal to 1° 24′. Similarly I obtained for the longitude of node, though not so certainly, 103°. For the preliminary search I determined to fix the latitude-limits of the zone at a width of one degree to the north and one degree to the south of this adopted plane. To these elements I strictly adhered, with the intention, however, of alternately increasing and decreasing the inclination, and varying the longitude of node if I should arrive at no successful result from the search of this limited zone.

I may remark that the detailed plan of the instrumental search had been completely digested and written out as early as the 5th of September. To assist in a decision as to what method of search I should employ, I had recourse to an inductive consideration of the real diameters of the known planets of the solar system. I arrived at the result that a diameter of 50,000 miles might be taken as the minimum value for a planet next beyond Neptune. On this assumption, the mean distance of 52·0 gave for its apparent diameter 2″.1. I did not, therefore, hesitate in adopting the method of search depending upon the detection of the planet by contrast of its disk and light with the appearance of an average star of about the thirteenth magnitude. In the actual search, a power of 600 was often employed, but most of the search was conducted with a power of 400 diameters.

On thirty clear, moonless nights, between the 3d of November, 1877, and the 5th of March, 1878, this search was carried on after the manner I have indicated.

After the first few nights I was surprised at the readiness with which my eye detected any variation from the average appearance of a star of a given faint magnitude: as a consequence whereof my observing book contains a large stock of memoranda of suspected objects. My general plan with these was to observe with a sufficient degree of accuracy all suspected objects. On the succeeding night of observation these objects were re-observed; and, at an interval of several weeks thereafter this observation was again verified. At 3 A. M., the 6th of March, 1878, the search was discontinued—my observing book ends with the following note:

"The adopted plane of orbit of trans-neptunian planet is now searched (without break) from

$$v=146.8°$$
$$\text{to } v=186.1.°"$$

I have much confidence in this telescopic search—my aim was to sweep the zone so carefully that there should be no pressing need of duplicating it.

I ought not to conclude this paper without adverting to the apparently long delay of its publication. From the very beginning I had approached the entire problem of search for a trans-neptunian planet with resolute direction toward the end which I regarded of the highest scientific import—that of *finding the possible planet at the earliest moment;* if I were successful, observations of its position would then be secured at once, and an accurate determination of its elements would be a matter of earlier realization—it seeming improbable that any prior chance observation would ever be brought to light. After pursuing the theoretic side of the question for a short time, I saw clearly that many years must elapse before the perturbing action of this body on any interior planet would afford anything like pronounced evidence of its existence; recourse must be had to the practical telescopic search. So I tarried longer with the residuals of Uranus only in the hope of a possible shortening of the search by some indication that the planet was more probably in one portion of the heavens than in another. After the telescopic search, which I was conducting, had been temporarily brought to an end, by circumstances beyond my control, I was not without hope of effecting some arrangement whereby I might resume the search at an early day, and carry it to a satisfactory conclusion. After much thought upon the apathetic reception with which the magnificent researches of Adams and LeVerrier had met, I reached the conclusion that no competent observer would be led to continue the search through knowledge of the little work of speculation that I had done. And, as the work was undertaken with the end always in view of finding the planet, it did not appear that any advantage would result from its publication.

It will be remarked that this matter now assumes a very different aspect: the publication of a recent memoir *On Comets and Ultra-Neptunian Planets*, by Professor George Forbes, of Glasgow, assigns, by a method of investigation entirely independent of my own, a position to a possible trans-neptunian planet which may be regarded as in exact coincidence with that which I have deduced. The assumption of a mean distance 100, indicated in Professor Forbes' paper, will not appreciably destroy the representation of the residuals with which I have dealt. I have not yet been able to convince myself that the remarkable harmony of the results of the two investigations is simply a chance agreement; and, with the hope that the accumulated evidence of the existence of a far exterior planet may not fail to incite some observer in possession of sufficiently powerful telescopic means to a vigorous prosecution of the search, I have prepared this preliminary paper in order that attention may be called to the matter in sufficient advance of the opposition-time now approaching. I may add here, that, should a careful and protracted search of the region adjacent to the indicated longitude prove unavailing, no more certain test of the existence of a trans-neptunian planet admits of application within the next few years than that of telescopic search of a limited zone extending entirely around the heavens—a search which I have been hoping, for more than two years past, for an opportunity to undertake, but which I see no present prospect of realizing.

NAUTICAL ALMANAC OFFICE, Washington, August 4, 1880.

IN the province of Keen-chang, China, $15,000,000 worth of a peculiar vegetable wax is annually produced. It is formed on the twigs of an evergreen tree (Ligustrum lucidum), whose oval leaves furnish homes for myriads of insects. These, during the spring, produce a thin skin over the leaves, from which exudes a waxy substance that hardens in the month of August. The twigs are then cut and boiled in water, by which means the purified wax is easily separated.

AN IMPROVED ELECTRO-MOTOR.*

By THEODORE WIESENDANGER.

While recently many minds have been at work, with more or less of success, to produce improvements in dynamo-generators of electric energy, very few have given their special care and attention to the development of the electro-motor. Experience has taught us hitherto that the efficiencies of one and the same machine for action and reaction, or for use either as a generator, or by the inverted process as an electro-motor, stand in a certain and direct proportion to each other, or that our most efficient generators, such as the Siemens, Brush and Gramme, machines prove also the most effective motors, and on the other hand that inferior dynamo-machines invariably are inefficient motors. It would, however, be hazardous to conclude from these results that this rule should hold good for all future machines, and from the results of researches I have recently made, I come to the conclusion that the motors which are to supersede those now in use could not be employed as generators. Dynamo-machines, such as now constructed, only prove efficient when their field-magnets are able to retain at all times (*e. g.*, even when the machine stands at rest) a certain and very considerable amount of residual magnetism, and for that reason their cores are made of retentive material, hard cast iron, as is the case in the Brush and Gramme machines ; or if the cores consist of soft iron they are attached to large masses of hard cast iron. in such a manner that the latter are inclosed in the magnetic circuit, and form part of the cores.

Generators of the same kind, when made small in size, have cores much larger and heavier in proportion, and, moreover, the baseplate, or, as in the Weston machine, a heavy retentive cylinder, is made to form a portion of the field-magnets. But all efforts hitherto made to produce efficient small dynamo-machines with cores of soft iron only, have resulted in absolute failure, although men of the highest genius have made repeated and prolonged efforts to solve that most difficult of problems.

These curious facts conclusively prove that the theory explanatory of the action of dynamo-machines, as now universally adopted, viz., the theory of inductive action and reaction between the field-magnets and the armature, cannot any longer be considered complete or satisfactory ; for even wrought iron, especially when occurring in large masses, always contains an appreciable amount of residual magnetism, more especially after it has once been subjected to strong magnetization, and if the above theory were correct and complete, then the smallest possible amount of residual magnetic energy, augmented by repeated action and reaction, would be sufficient for the starting of such a machine to action. This, however, experience proves *not* to be the case, and the theory, although stoutly adhered to, must be either abandoned or amended.

The inventors of the most recent electro-motive engines have worked—perhaps unconsciously—upon the idea that the construction and action of electro-motors are based altogether upon the same laws as those of dynamo or magneto machines, and, in accordance with that assumption, the field-magnets of the Desprez motor are made to consist of large and heavy masses of magnetized steel.

Experimenters have also for a long time past clung to the idea that the efficiency of an electro-motor—or the amount of energy to be obtained from such a machine by means of a current of given strength circulating in the coils of its armature only—bears a definite and direct proportion to the magneto-inductive power of its field-magnets, and that an increase of power in the field-magnets alone must necessarily produce greater capabilities of the machine.

This, however, is a mischievous theory, because erroneous in its very principles, and development would only lead to the hypothesis of perpetual motion. On the contrary, starting from the consideration of the fact that a very small magnetic needle, if acted upon by one of the poles of another and very powerful magnet, has its polarity destroyed or reversed, and that if one of its poles, say the N pole, is presented to a similar (N) pole of the large magnet, the former will instantly lose its characteristic qualities and be attracted by its overpowering opponent, we can only come to the one rational conclusion that the power or the field-magnets of an electro-motor, as compared to that of the magnet or magnets constituting its armature, should not surpass the limit of some certain ratio, to be determined yet by experiments carefully conducted, and that, if it surpasses the limit, the capabilities of the machine must be impaired. Acting on this principle, I have constructed a motor in which the power of the field magnets is as nearly as possible equal to that of an armature, the core of the former being very light and made entirely of soft iron ; and the satisfactory results obtained from this machine are a sure sign that further investigation of the subject and experiments made with a view of determining the exact ratio of power between the magnets and armature will result in further improvement.

Another and very important consideration in the construction of dynamo-machines and electro-motors has not yet received that care and attention from scientific investigators which would lead to immediate progress. It is the method of motion of the revolving armature with regard to its approaching to, or receding from the poles of the field-magnets. In nearly all the machines now constructed the polar faces of the cores of the field-magnets and those of the armature are of such a shape, and the latter is caused to revolve in such a manner, that only in a small portion of the revolution its poles either approach the poles of the field-magnets or recede from them. But the most successful production of induced currents will be achieved, and the greatest amount of power will be derived from a motor, if attention is paid not merely to the *one* condition, that the armature should revolve in the most highly concentrated magnetic field possible, but also that nearly the entire motion of the revolving armature should be either one of approach or of withdrawal. Let us first of all consider the case of a machine with two poles only of field-magnets and two poles of the revolving armature.

It is usual to give the active faces of the former such a shape that a section of the same represents a portion of a true circle. See Fig. 7.

In the ordinary machines now in use the radius of the circle described by the outline of the revolving armature, and that of the larger circle described in portion by the section of the inner or active faces of the poles are nearly the same, and the two circles are concentric. (See Fig. 7.) The pole *g* of the armature only approaches the pole A of the field-magnets while moving from *c* to *d*, or where the intensity of the magnetic field of A is at its minimum. When continuing its motion from *d* to *r* and to *f*, the pole *g* can no longer be said to approach A, because the distance between the respective surfaces remains constant.

I therefore propose that the devices shown in Figs. 9 and 6 should be adopted. The radius of the circle, part of which is formed by the section *d*, *r*, *c*, is considerably larger than that of the circle described by the outline of the field of motion of the armature ; *d*, *r*, *c* is, moreover, considerably less than the half of a circle and the three circles *d*, *r*, *c*, *f*, *e*, *h*, and that described by the outline of the field of motion of the armature are not concentric. The pole *g* of the armature, when in motion, approaches the pole, A, not only in its course from *e* to C, but also when in the most intense magnetic field of A, viz., whilst moving from C to *z* and *d*. Fig. 11 represents a section of the field-magnet's cones E F and G H, and pole's pieces N and S cast in two halves and mounted on a base board, to which they are fixed by the two bolts R and T. The same principles may be applied to machines with field-magnets of more than two poles (see Fig. 3) ; or the armature itself may be made of such a shape as to work under the conditions above stated (Fig. 4). But even if the poles of the armature and those of the field-magnets are of the ordinary shape, a machine with more magnets will be more perfect in its action than one with two poles only. Fig. 10 illustrates a machine in which the armature during nearly the whole of its motion either approaches to or recedes from

*A paper read before the British Association.

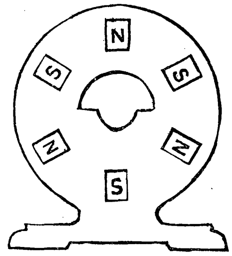

FIG. 1.

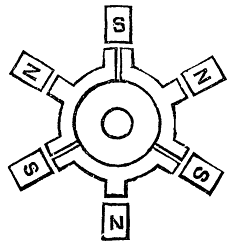

FIG. 2.

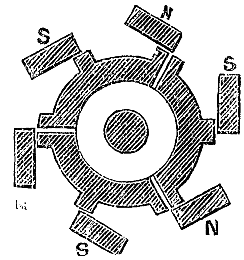

FIG. 3.

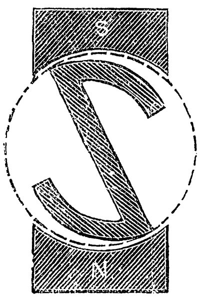

FIG. 4.

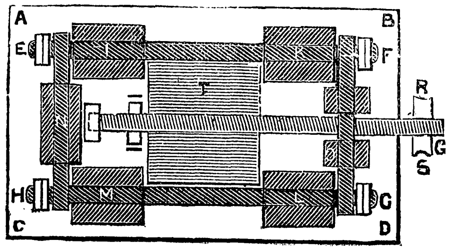

FIG. 5.

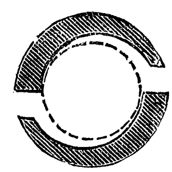

FIG. 6.

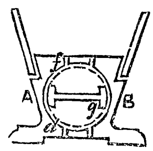

FIG. 7.

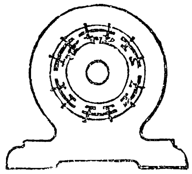

FIG. 8.

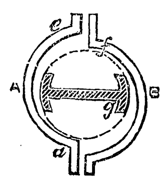

FIG. 9.

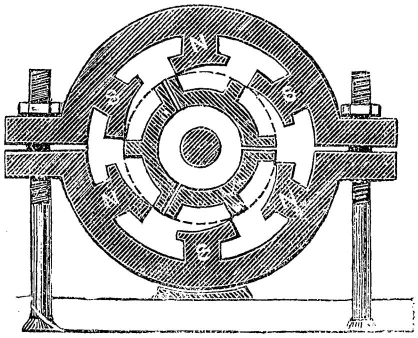

FIG. 10.

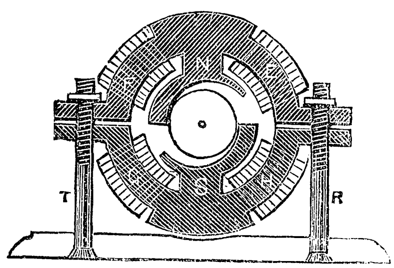

FIG. 11.

WIESENDANGER'S IMPROVED ELECTRO-MOTOR.

the pole of the field-magnets. In such machines the motion of the poles of the armature is also more in a line, coincident with the line of attraction as exercised between the two systems of poles; while in machines with field-magnets of two poles only the motion of the poles of the armature is at times at angles of 45 degrees to one degree from the direct pull.

I may, perhaps, be allowed to call attention to another matter of importance, awaiting further research. We find that in the three types of dynamo-machines, as constructed by Siemens, Gramme and Wilde, the relative positions of the axes of the field-magnets and those of the armatures are altogether different. Yet the three systems work well. We are unable, however, to state with certainty which positions of the axes are the best, or why any one of these positions should be better than the others, and in the face of experience, the theory of tubes or lines of force is little more than a hypothesis, with all its diffusion, vagueness and uncertainty.

Having so far considered general principles chiefly, I now beg to describe this motor (Fig. 5). A B C D is a wooden baseboard, E F G H a frame, consisting of the two parallel round rods F E and G H, and the two flat bars F G and E H, made of the best wrought iron, and carefully softened. The four bars are screwed together at the corners, and supported by four brass brackets over the baseboard. These inner rods form the compound core of the field magnets, a combination, as it were, of two horseshoe magnets, whose similar poles (S S and N N) form the junctions. Thus we have practically two poles only, a S and a N pole. Six coils of insulated copper wire are wound over the different portions of this core, shown in the drawing; the active polepieces are left exposed for a long distance, bearing no coils, The spindle P L, which carries a Siemens armature of the old form, or an armature with a compound tubular core; the commutator and pulley traverses the flat crossbar F H. The core of the armature is made of sheets of charcoal-iron, and it bears a coil of stout insulated copper wire. The commutator is of the ordinary kind, consisting of two half-tubes of brass, insulated from each other and from the spindle, and each forming one of the terminals of the coil. Fig. 2 represents a sectional view of a compound machine, acting on the same principles; Fig. 1 is a view of the two-end castings which hold the field-magnet. This machine contains a system of six field-magnets and six poles, and a compound armature with six poles. The current is to be reversed six times for each revolution, and to accomplish this I have devised the following commutator (see Fig. 8): —In these machines, also, the poles of the field-magnets or those of the armature may be of such a shape as to be nearly always approaching to, or receding from, each other, while in active motion,

The development of most important machines is destined to reach a certain stage of perfection, when further improvements cannot be accomplished by the inventor unaided; the second and important factor needed then is the co-operation of inventive and investigative talent with capital, This stage of perfection has been reached in the steam-engines, gas-engines, printing-machinery, etc., and it may be said to be rapidly approached by the progress made in dynamo-machines and electro-motors.

The development of the latter machines is followed by the scientific world with greater interest, and it evokes more eager expectations than that of other machinery, chiefly because it is not, and cannot be, identified with the solution of a problem limited within the confines of mechanical difficulties and commercial interests, but necessitates a further and deeper investigation into that great and subtle power, electricity, whose manifestations are so striking in their effects, so mysterious in their nature, so promising of great results in an immediate future, so fertile a field of research to the pioneer of science.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE. 1880.

THE British Association for the Advancement of Science met at Swansea on the 4th of September last, under the presidency of Dr. Ramsey, who took as the subject of his address, "The Recurrence of Certain Geological Phenomena in Geological Time." His object appeared to be to show that all known geological formations have been produced under physical circumstances closely resembling, if not identical, with those with which we are more or less familiar. Through the various geological epochs he traced this identity of operations in respect to the metamorphism of rocks, the products of volcanoes, the upheaving and denudations of mountain chains, the deposit of great *inland areas* of salt, a recurrence of fresh-water conditions in lakes and estuaries, and glacial influences. His conclusion was that from the Laurentian epochs down to the present day all physical events in the history of the earth have varied neither in kind nor in intensity from those of which we now have experience.

The conclusions drawn from this address are summed up in the closing words of Professor Ramsey's discourse, as follows:

"In opening this address, I began with the subject of the oldest metamorphic rocks that I have seen—the Laurentian strata. It is evident to every person who thinks on the subject that their deposition took place far from the beginning of recognized geological time. For there must have been older rocks by the degradation of which they were formed. And if, as some American geologists affirm, there are on that continent metamorphic rocks of more ancient dates than the Laurentian strata, there must have been rocks more ancient still to afford materials for the deposition of these pre-Laurentian strata. Starting with the Laurentian rocks, I have shown that the phenomena of metamorphism of strata have been continued from that date all through the later formations, or groups of formations, down to and including part of the Eocene strata in some parts of the world. In like manner I have shown that ordinary volcanic rocks have been ejected in Silurian, Devonian, Carboniferous, Jurassic, Cretaceo-oolitic, Cretaceous, Eocene, Miocene, and Pliocene times, and from all that I have seen or read of these ancient volcanoes, I have no reason to believe that volcanic forces played a more important part in any period of geological time than they do in this our modern epoch. So, also, mountain chains existed before the deposition of the Silurian rocks, others of later date before the Old Red Sandstone strata were formed, and the chain of the Ural before the deposition of the Permain beds. The last great upheaval of the Alleghany Mountains took place between the close of the formation of the Carboniferous strata of that region and the deposition of the New Red Sandstone. According to Darwin, after various oscillations of level, the Cordillera underwent its chief upheaval after the Cretaceous epoch, and all geologists know that the Alps, the Pyrenees, the Carpathians, the Himalayas, and other mountain chains (which I have named) underwent what seems to have been their chief great upheaval after the deposition of the Eocene strata, while some of them were again lifted up several thousands of feet after the close of the Miocene epoch. The deposition of salts from aqueous solutions in inland lakes and lagoons appears to have taken place through all time—through Silurian, Devonian, Carboniferous, Permian, Triassic, Jurassic, Cretaceous. Eocene, Miocene, and Pliocene epochs—and it is going on now. In like manner fresh-water and estuarine conditions are found now in one region, now in another, throughout all the formations or groups of formations possibly from Silurian times onward; and glacial phenomena, so far from being confined to what was and is generally still termed *the* Glacial Epoch, are now boldly declared by independent witnesses of known high reputation, to begin with the Cambrian epoch, and to have occurred somewhere, at intervals, in various formations, from almost the earliest Palæozoic times down to our last post-Pliocene "Glacial Epoch."

If the nebular hypothesis of astronomers be true (and I know of no reason why it should be doubted), the earth was at one time in a purely gaseous state, and afterwards in a fluid condition, attended by intense heat. By and by consolidation, due to partial cooling, took place on the surface, and as radiation of heat went on the outer shell thickened. Radiation still going on, the interior fluid matter decreased in bulk, and, by force of gravitation, the outer shell being drawn towards the interior, gave way, and, in parts, got crinkled up, and this, according to cosmogonists,

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was the origin of the earliest mountain chains. I make no objection to the hypothesis, which, to say the least, seems to be the best that can be offered, and looks highly probable. But, assuming that it is true, these hypothetical events took place so long before authentic geological history began, as written in the rocks, that the earliest of the physical events to which I have drawn your attention in this address was, to all human apprehension of time, so enormously removed from these early assumed cosmical phenomena, that they appear to me to have been of comparatively quite modern occurrence, and to indicate that from the Laurentian epoch down to the present day, all the physical events in the history of the earth have varied neither in kind nor in intensity from those of which we now have experience. Perhaps many of our British geologists hold similar opinions, but if it be so, it may not be altogether useless to have considered the various subjects separately on which I depend to prove the point I had in view."

MATHEMATICS AND PHYSICS.

The address was delivered by the president, Prof. W. Grylls Adams. In it he dealt with the subject of magnetic disturbances, and pointed out that in many instances the disturbances at the various stations of observations were not precisely alike, showing probably the change of the direction or intensity of the earth's magnetism arising from the solar action upon it. He believed there was a sufficient cause for all our terrestrial magnetic changes, for these masses of metal were ever boiling up from the lower and hotter levels of the sun's atmosphere to the cooler upper regions, where they must again form clouds to throw out their light and heat, and to absorb the light and heat coming from the hotter lower regions; then they became condensed and were drawn again back towards the body of the sun, so forming those remarkable dark spaces or sunspots by their down-rush towards the lower levels. In these vast changes, which we know from the science of energy must be taking place, but of the vastness of which we can have no conception, we have abundant cause for the magnetic changes which we observe at the same instant at distant points on the surface of the earth, and the same cause acting by induction on the magnetic matter within and on the earth may well produce changes in the direction of its total magnetic force, and alter the direction of its magnetic axis. These magnetic changes on the earth will influence the declination needles at different places, and will cause them to be deflected. The direction of the deflection must depend on the situation of the earth's magnetic axis, or the direction of its motion with regard to the stations where the observations are made. Thus, both directly and indirectly, we find in the sun not only the cause of diurnal magnetic variations, but also the cause of these remarkable magnetic changes and disturbances over the surface of the earth.

CHEMISTRY.

The address was delivered by the president, Dr. J. H. Gilbert, F.R.S., who referred mainly to the subject of agricultural chemistry, and in the course of his remarks said, referring to the assimilation of carbon, that the whole tendency of observations was to conform to the opinion put forward by De Saussure about the commencement of the century, and so forcibly insisted upon by Liebig, forty years later, that the greater part, if not the whole, of the carbon, was derived from the carbonic acid of the atmosphere. Judging from more recent researches, it would seem probable that the estimate of one part of carbon or carbonic acid in 10,000 of air was more probably too high than too low as an estimate of the average quantity in the atmosphere of our globe. Large as was the annual accumulation of carbon from the atmosphere over a given area, it was obvious that the quantity must vary exceedingly with the variation of climatical conditions. It was, in fact, several times as great in the case of the tropical vegetation—that of the sugar-cane, for instance. And not only was the greater part of the assimilation accomplished within a comparatively small portion of the year, but the action was limited to the hours of daylight, whilst during darkness there was rather loss than gain. In a general sense it might be said that the success of the cultivator might be measured by the amount of carbon he succeeded in accumulating in his crops. And as the amount of carbon accumulated depended on the supply of nitrogen in an available form within the reach of plants, it was obvious that the question of the sources of the nitrogen of vegetation was one of first importance. The result of experiments that had been conducted went to prove—first, that without nitrogenous manure, the gramineous crops annually yielded, for many years in succession, much more nitrogen over a given area than was accounted for by the amount of combined nitrogen annually coming down in the measured aqueous deposits from the atmosphere; second, the roots yielded more nitrogen than the cereal crops, and the leguminous crops much more still; and third, that in all cases—whether of cereal crops, root crops, leguminous crops, or a rotation of crops—the decline in the annual yield of nitrogen, when one was supplied, was very great. The next point referred to was the condition of the nitrogen in our various crops. They could not say that the whole of the nitrogen in the seeds with which they had to deal existed as albuminoids. But they might safely assume that the nearer they approached to perfect ripeness, the less of non-albuminoid nitrogenous matters would they contain; and in the case of the cereal grains, at any rate, it was possible that if really perfectly ripe, they would contain very nearly the whole of their nitrogen as albuminoids.

GEOLOGY.

The address was delivered by the President, H. C. Sorly, LL.D., F.R.S., who took for his subject the comparative structure of artificial slags and eruptive rocks. His conclusions may be thus summed up:

The objects I have described may be conveniently separated into three well-marked groups, viz.: artificial slags, volcanic rocks and granite rocks. My own specimens all show perfectly well-marked and characteristic structures though they are connected, in some cases, by intermediate varieties. Possibly, such connecting links might be more pronounced in other specimens that have not come under my notice. In any case, the facts seem abundantly sufficient to prove that there must be some active cause for such a common, if not general, difference in the structural character of these three different types. The supposition is so simple and attractive, that I feel very much tempted to suggest that this difference is due to the presence or absence of water as a gas or as a liquid. In the case of slags it is not present in any form. Considering how large an amount of steam is given off from erupted lavas, and that, as a rule, no fluid cavities occur in the constituent minerals, it appears to me very plausible to suppose that those structures which are specially characteristic of volcanic rocks are, in great measure, if not entirely, due to the presence of associated or dissolved vapor. The fluid cavities prove that water was sometimes, if not always, present as a liquid during the consolidation of granitic rocks, and we can scarcely hesitate to conclude that it must have had very considerable influence on the rock during consolidation. Still, though these three extreme types appear to be thus characterized by the absence of water, or by its presence in a state of vapor or liquid, I think we are scarcely in a position to say that this difference in the conditions is more than a plausible explanation of the differences in their structure. Confining our attention to the more important crystalline constituents which are common to the different types, we may say that that the chief structural characters of the crystals are as follows: (*a*) Skeleton crystals, (*b*) Fan-shaped groups, (*c*) Glass cavities, (*d*) Simple crystals, (*e*) Fluid cavities. These different structural characters are found combined in different ways in the different natural and artificial products, and for simplicity I will refer to them by means of the affixed letters. The type of the artificial products of fusion may generally be expressed by $a + b$ or $b + c$; that is to say, it is characterized by skeleton crystals and fan-shaped groups, or by fan-shaped groups and glass cavities. In like manner the volcanic group may be expressed occasionally by $b + c$, but generally by $c + d$, and the granitic by $d + e$. These relations will be more apparent if given in the form of a table as follows:

Slag type........	$a + b$	$b + c$	
Volcanic type.. ..	$b + c$	$c + d$	
Granitic type			$d + e$

Hence it will be seen that there is a gradual passage from one type to the other by the disappearance of one character and the appearance of another, certain characters in the meanwhile remaining common, so that there is no sudden break, but an overlapping of structural characteristics. It is, I think, satisfactory to find that, when erupted rocks are examined from such a new and independent point of view, the general conclusions to which I have been led are so completely in accord with those arrived at by other methods of study.

ANATOMY AND PHYSIOLOGY.

The address was delivered by Mr. F. M. Balfour, F.R.S., one of the vice-presidents of the section, who observed that in the spring of the present year Prof. Huxley delivered an address at the Royal Institution, to which he gave the felicitous title of "The Coming of Age of the Origin of Species." It was, as Prof. Huxley pointed out, twenty-one years since Mr. Darwin's great work was published, and the present occasion, Mr. Balfour remarked, was an appropriate one to review the effect which it had had on the progress of biological knowledge. There was, he might venture to say, no department of Biology the growth of which has not been profoundly influenced by the Darwinian theory. When Messrs. Darwin and Wallace first enunciated their views to the scientific world, the facts they brought forward seemed to many naturalists insufficient to substantiate their far-reaching conclusions. Since that time an overwhelming mass of evidence has, however, been rapidly accumulating in their favor. Facts which at first appeared to be opposed to their theories have one by one been shown to afford striking proofs of their truth. There are at the present time but few naturalists who do not accept in the main the Darwinian theory, and even some of those who reject many of Darwin's explanations still accept the fundamental position, that all animals are descended from the common stock. To attempt in the time at his disposal to trace the influence of the Darwinian theory on all the branches of anatomy and physiology would be wholly impossible, and he would confine himself to an attempt to do so for a small section only. There was perhaps no department of Biology which had been so revolutionized by the theory of animal evolution as that of development or Embryology. The reason of this is not far to seek. According to the Darwinian theory, the present order of the organic world has been caused by the action of two laws, known as the laws of heredity and of variation. The law of heredity is familiarly exemplified by the well-known fact that offspring resemble their parents. Not only, however, do the offspring belong to the same species as their parents, but they inherit the individual peculiarities of their parents. It is on this that the breeders of cattle depend, and it is a fact of every-day experience amongst ourselves. A further point with reference to heredity to which he must call their attention was the fact that the characteristics which display themselves at some special period in the life of the parent are acquired by the offspring at a corresponding period. Thus, in many birds the males have a special plumage in the adult state. The male offspring is not, however, born with the adult plumage, but only acquires it when it becomes adult. The law of variation is, in a certain sense, opposed to the law of heredity. It asserts that the resemblance which offspring bear to their parents is never exact. The contradiction between the two laws is only apparent. All variations and modifications in an organism are directly or indirectly due to its environments; that is to say, they are rather produced by some direct influence acting upon the organism itself, or by some more subtle and mysterious action on its parents; and the law of heredity really asserts that the offspring and parent would resemble each other if their environments were the same. Since, however, this is never the case, the offspring always differ to some extent from the parents. Now, according to the law of heredity, every acquired variation tends to be inherited, so that, by a summation of small changes, the animals may come to differ from their parent stock to an indefinite extent. Mr. Balfour then referred to what he spoke of as a concrete example of the application of these two laws, his object being to demonstrate how completely modern embryological naming is dependent on inheritance and variation, which constitute the keystones of the Darwinian theory. He maintained that "The Origin of Species" afforded explanations of important embryological facts, and added that no explanation, for instance, could be offered of the fact that a frog in the course of its growth has a stage in which it breathes like a fish, and then why it is like a newt with a long tail, which gradually becomes absorbed, and finally disappears. To the Darwinian the explanation of such facts is obvious. The stage when the tadpole breathes by gills is a repetition of the stage when the ancestors of the frog had not advanced in the scale of development beyond a fish, while the newt-like stage implies that the ancestors of the frog were at one time organized very much like the newts of to-day. The explanation of such facts has opened out to the embryologist quite a new series of problems. Having examined these in regard to phylogeny and organogeny, and entering into elaborate scientific details and arguments, Mr. Balfour concluded by remarking that although the present state of our knowledge on the genesis of the nervous system is a great advance on that of a few years ago, there is still much remaining to be done to make it complete. The subject, he urged, was well worth the attention of the morphologist, the physiologist, or even the psychologist, and we must not remain satisfied by filling up the gaps in our knowledge by such hypotheses as he had been compelled to frame. New methods of research will probably be required to grapple with the problems that are still unsolved; but when we look back and survey what has been done in the past, there can be no reason for mistrusting our advance in the future.

RELATION OF VERMONT ARCHÆOLOGY TO THAT OF THE ADJACENT STATES.*

By Dr. George H. Perkins.

Vermont is a very barren region archæologically as compared with many parts of the West, yet thorough investigation has shown that even there interesting results may be obtained. We not only have found a not inconsiderable number of stone relics, but we have also found, as we think, an interesting relation between these specimens and those from surrounding States. West of the Green Mountains we find our greatest variety of objects, and we find at least two classes, and perhaps more, which should be referred to different people. Here and there, but especially near Lake Champlain, we find objects of copper, and polished stone much more skillfully made than most of the specimens found in New England. In certain graves found near Swanton, and described fully at the Portland meeting of this Association, we find this class of objects. A peculiar form of slate knife (or lance?), polished and with notched haft, is found in Western Vermont, but occurs in greater abundance across the lake in New York and in Central New York. At Palatine Bridge Mr. S. L. Frey has discovered graves of the same kind as those found at Swanton. Taking these finer specimens of ancient workmanship as a basis of comparison, leaving out of account the ruder stone objects and the pottery, we can duplicate most of our Vermont specimens in Central New York, and also we find from Western New York and the mounds of Ohio many which are identical in all essential characters. This is true of shell and copper beads, of copper spear-heads, of stone tubes, axes, gorgets, banner stones and other objects. As we go westward we find these specimens increasing in number and of greater variety, and we also find a few forms absent. These specimens seem to me sufficiently characteristic and numerous to warrant the inference that in them we have a record of a people who emigrated from Ohio through New York, crossed Lake Champlain and reached as far east as the Green Mountains, where they stopped. They also appear not to have reached further north than Northern Vermont, nor further south than the southern end of Lake Champlain.

The other class of relics is composed of ruder objects associated with pottery. So far as I know no pottery has been found with the first class of relics. This pottery is quite unlike that from the mounds or most of that found

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THE INDIAN CENSUS.*

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It is also probable that the interest in ethnologic research, noticeable throughout the country, has influenced Congress. General Walker, the able superintendent of the census, has availed himself of an agency that never before existed. The Bureau of Ethnology, lately established by act of Congress and now under the direction of Major Powell, was entrusted with the whole of the duty in question. Without the preparation already made by the Bureau of Ethnology the work could not be done accurately, and by scientific methods. It might possibly have fallen into the hands of mere office seekers, perhaps of persons interested in the concealment if not perpetration of frauds. The enumeration of the Indians is difficult. Though restrained more or less successfully within specified limits, they are still apt to range over large regions, and to be away, for long periods from the place of their compulsory or voluntary habitation. This is especially the case in Summer, and the day of June fixed for the general census being inappropriate, the first day of October was selected instead. There are other causes interfering with accuracy. If fraud is attempted it is assisted by an enlarged paper-number of recipients of rations, and the Indians themselves are tempted to swell their lists, both for rations and annuities. Hostile or troublesome bands, under differing circumstances, seek to exaggerate or conceal their military strength. The aboriginal reluctance of each person to give his own name, and of all to speak of deceased relatives and friends is well known. These and many other obstacles require that the duty shall be in charge of persons familiar with the Indian customs, who both know what to look for and how to find it. The forms and schedules of the general census being wholly inapplicable, others have been prepared with great care. They are five in number. 1. *Population.* Each sheet is confined to one family in one dwelling, that unit being of much greater importance in savage and barbaric than in civilized life. The location of the dwelling is given by legal and natural subdivisions, also its description; if a house, whether of brick, stone, adobe, frame or log; if pueblo, whether stone or adobe; if lodge, whether of cloth, skins, slabs, poles, brush, bark, tule, stone or earth. The head of the family, often a woman, is first designated, and the relationship of each person to that head. For each individual the Indian name is given, with the English translation of that name; also the English, Spanish, French or other name habitually used. This serves not merely for identification, but brings out the names originally designated on the system of the *gens* organization, and also the title or sobriquet generally bestowed in after-life from some achievement or circumstance often of sociologic, if not historic, interest. Mixture of blood between several tribes, and between Indians and whites and negroes, is noted, and all matters relating to advance in civilization, such as wearing citizen's dress, amount and kind of personal and real property ownership, in which is recognized cultivation of land and sources of subsistence. 2. The schedule for *vital statistics* inquires into the causes of deaths during the past year, and the prevalence of the diseases to which Indians are subject; among other interesting points obtaining in the Indian tongue a statement from the head of the family, or medicine man, of the cause of death, thus showing the aboriginal theories of diseases. 3, *Industries*, embraces every appropriate particular under that head, classified for full and mixed bloods, and adopted whites and negroes, all by tribes instead of by families and individuals, as in the "population" schedule, and with details more useful for statistical purposes. 4, *Education*, is on the same principle. Schedule 5 guides and simplifies research into the wondrous system of ramified consanguinities and affinities, on which savage society is founded and depends. The work of the present census of the Indians will be of great practical value. 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EXPERIMENTS ON THE STRENGTH OF YELLOW PINE.*

By Prof. R. H. Thurston.

The elasticity of yellow pine timber as used in construction is very variable, the modulus varying from one to three millions, the average being about two millions in small sections, and a little above one and a half millions in large timber.

The highest values are as often given by green as by seasoned timber, and that, under sixteen square inches section and fifty-four inches length, at least, the magnitude of the modulus of elasticity is independent of the size of the piece.

The density of the wood does not determine the modulus; since the figure varies sometimes directly and sometimes inversely with the density, even where the wood is as nearly as possible in the same condition as to seasoning.

A high modulus usually accompanies high tenacity and great transverse strength, but it is not invariably the fact that maximum ultimate strength is accompanied by initial stiffness.

The pseudo moduli, determined by taking considerable deflections, are usually not greatly different from those determined from small deflections and light loads. The values of these moduli often decrease with increase in deflection.

An inspection of the woods tested plainly indicates, in the opinion of the writer, that the density of the pines is so considerably modified by the amount of pitch contained in the sap channels that it cannot be regarded as indicative of the strength of the timber. Where quite free from sap the wood usually exhibits increase of strength and elastic resistance to deflection, with increase of density.

The strength of timber, otherwise similar, is greatly affected by its structure, and the resistance offered to stresses applied transversely is greatest when the sections

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THE INDIAN CENSUS.*

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It is also probable that the interest in ethnologic research, noticeable throughout the country, has influenced Congress. General Walker, the able superintendent of the census, has availed himself of an agency that never before existed. The Bureau of Ethnology, lately established by act of Congress and now under the direction of Major Powell, was entrusted with the whole of the duty in question. Without the preparation already made by the Bureau of Ethnology the work could not be done accurately, and by scientific methods. It might possibly have fallen into the hands of mere office seekers, perhaps of persons interested in the concealment if not perpetration of frauds. The enumeration of the Indians is difficult. Though restrained more or less successfully within specified limits, they are still apt to range over large regions, and to be away, for long periods from the place of their compulsory or voluntary habitation. This is especially the case in Summer, and the day of June fixed for the general census being inappropriate, the first day of October was selected instead. There are other causes interfering with accuracy. If fraud is attempted it is assisted by an enlarged paper-number of recipients of rations, and the Indians themselves are tempted to swell their lists, both for rations and annuities. Hostile or troublesome bands, under differing circumstances, seek to exaggerate or conceal their military strength. The aboriginal reluctance of each person to give his own name, and of all to speak of deceased relatives and friends is well known. These and many other obstacles require that the duty shall be in charge of persons familiar with the Indian customs, who both know what to look for and how to find it. The forms and schedules of the general census being wholly inapplicable, others have been prepared with great care. They are five in number. 1. *Population.* Each sheet is confined to one family in one dwelling, that unit being of much greater importance in savage and barbaric than in civilized life. The location of the dwelling is given by legal and natural subdivisions, also its description; if a house, whether of brick, stone, adobe, frame or log; if pueblo, whether stone or adobe; if lodge, whether of cloth, skins, slabs, poles, brush, bark, tule, stone or earth. The head of the family, often a woman, is first designated, and the relationship of each person to that head. For each individual the Indian name is given, with the English translation of that name; also the English, Spanish, French or other name habitually used. This serves not merely for identification, but brings out the names originally designated on the system of the *gens* organization, and also the title or sobriquet generally bestowed in after-life from some achievement or circumstance often of sociologic, if not historic, interest. Mixture of blood between several tribes, and between Indians and whites and negroes, is noted, and all matters relating to advance in civilization, such as wearing citizen's dress, amount and kind of personal and real property ownership, in which is recognized cultivation of land and sources of subsistence. 2. The schedule for *vital statistics* inquires into the causes of deaths during the past year, and the prevalence of the diseases to which Indians are subject; among other interesting points obtaining in the Indian tongue a statement from the head of the family, or medicine man, of the cause of death, thus showing the aboriginal theories of diseases. 3, *Industries*, embraces every appropriate particular under that head, classified for full and mixed bloods, and adopted whites and negroes, all by tribes instead of by families and individuals, as in the "population" schedule, and with details more useful for statistical purposes. 4, *Education*, is on the same principle. Schedule 5 guides and simplifies research into the wondrous system of ramified consanguinities and affinities, on which savage society is founded and depends. The work of the present census of the Indians will be of great practical value. It will correct some popular errors which have obstructed judicious legislation, confused statesmanship and misled philanthropy, and will render frauds difficult of perpetration. The schedules also show that advantage has been taken of this opportunity to lead research into points of deep scientific interest.

*Read before the A. A. A. S., Boston, 1880.

EXPERIMENTS ON THE STRENGTH OF YELLOW PINE.*

By PROF. R. H. THURSTON.

The elasticity of yellow pine timber as used in construction is very variable, the modulus varying from one to three millions, the average being about two millions in small sections, and a little above one and a half millions in large timber.

The highest values are as often given by green as by seasoned timber, and that, under sixteen square inches section and fifty-four inches length, at least, the magnitude of the modulus of elasticity is independent of the size of the piece.

The density of the wood does not determine the modulus; since the figure varies sometimes directly and sometimes inversely with the density, even where the wood is as nearly as possible in the same condition as to seasoning.

A high modulus usually accompanies high tenacity and great transverse strength, but it is not invariably the fact that maximum ultimate strength is accompanied by initial stiffness.

The pseudo moduli, determined by taking considerable deflections, are usually not greatly different from those determined from small deflections and light loads. The values of these moduli often decrease with increase in deflection.

An inspection of the woods tested plainly indicates, in the opinion of the writer, that the density of the pines is so considerably modified by the amount of pitch contained in the sap channels that it cannot be regarded as indicative of the strength of the timber. Where quite free from sap the wood usually exhibits increase of strength and elastic resistance to deflection, with increase of density.

The strength of timber, otherwise similar, is greatly affected by its structure, and the resistance offered to stresses applied transversely is greatest when the sections

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of the timber taken transversely exhibit most nearly vertical lines of grain.

The modulus of rupture by transverse stress varies, for yellow pine, from $R = \frac{3}{2}\frac{Wl}{bd^2} = 10{,}000$ to 17,000, the highest values being usually obtained from well-seasoned wood. An average value may be taken as $R = 13{,}000$ for good timber, which in the formula $W = C\frac{bd^2}{l}$ gives $C = 866$ pounds or, practically, $W = 9000\frac{bd^2}{l}$ for good yellow pine.

The modulus of rupture varies as irregularly and with as little regard to size or density of the material as does the co-efficient for elasticity.

In the use of such materials, the only safe course for the designing and constructing engineer is evidently to adopt a moderate value of the modulus in proportioning his work, and by careful inspection and test to secure the rejection of all material which is not of good quality.

As has been seen, careful inspection may sometimes lead to the selection of material twenty-five per cent. superior to the average of good timber, and fifty per cent. more valuable than the lower grades such as are often sold in our markets.

The Paper was illustrated by a series of tabulated statements, being the result of experiments made to arrive at the conclusions prescribed in this abstract.

BOOKS RECEIVED.

Mortuary Customs Among the North American Indians.*

The primitive manners and customs of the North American Indians are rapidly passing away under influences of civilization and other disturbing elements. In view of this fact, it becomes the duty of all interested in preserving a record of these customs, to labor assiduously, while there is still time, to collect such data as may be obtainable. This seems the more important now, as within the last ten years an almost universal interest has been awakened in ethnologic research, and the desire for more knowledge in this regard is constantly increasing. A wise and liberal government, recognizing the need, has ably seconded the efforts of those engaged in such studies by liberal grants from the public funds; nor is encouragement wanted from the hundreds of scientific societies throughout the civilized globe. The public press, as the mouth-piece of the people, is ever on the alert to scatter broad-cast such items of ethnologic information as its corps of well-trained reporters can secure. To induce further laudable inquiry, and to assist all those who may be willing to engage in the good work, is the object of this preliminary work on the Mortuary customs of the North American Indians, and it is hoped that many more laborers may, through it, be added to the extensive and honorable list of those who have already contributed.

It would appear that the subject chosen should awaken great interest since the peculiar methods followed by different nations, and the great importance attached to burial ceremonies, have formed an almost invariable part of all works relating to the different peoples of our globe; in fact no particular portion of ethnologic research has claimed more attention.

In view of these facts it might seem almost a work of supererogation to continue a further examination of the subject; for nearly every author, in writing of our Indian tribes, makes some mention of burial observances; but these notices are scattered far and wide on the sea of this special literature, and many of the accounts, unless supported by corroborative evidence, may be considered as entirely unreliable. To bring together and harmonize conflicting statements, and arrange collectively what is known of the subject, has been the writer's task. This volume forms the third of a series, the first of which, entitled "Introduction to the Study of Indian Languages," was written by Major J. W. Powel, the director of the Bureau of Ethnology, Washington; the second being by Col. Garrick Mallery, and entitled, "Introduction to the Study of Sign-Language among the North American Indians."

The following provisional arrangement of burials has been adopted in arranging the facts presented in this work.

1. By Inhumation in pits, graves, holes in the ground, mounds, cists, and caves.

2. By Cremation, generally on the surface of the earth, occasionally beneath.

3. By Embalmment, or a process of mummifying, the remains being afterwards placed in the earth, caves, mounds or charnel house.

4. By Aerial Sepulture, the bodies being deposited on scaffolds, or trees, in boxes or canoes.

5. By Aquatic Burial, beneath the water or in canoes, which were turned adrift.

Major J. W. Powel gives the assurance that to those who are willing to take part in this work by earnest and faithful research, Dr. Yarrow will give full credit for their work in his final publication, and we would suggest that those able and willing to assist should put themselves in communication with the Bureau of Ethnology, Smithsonian Institution, Washington, and request instructions as to the best methods of recording their work.

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